Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

cal changes were found to be essentially the same as those observable on the stimulation of muscle. We find, then, that the advances in Physiology, like those in Anatomy, teach the essential unity of life in all living things, whether we call them animals or plants.

With this in our minds we may go on to consider in conclusion, and very briefly, that department of physiological study which is known as the Bionomics or Œcology of plants. In the earlier part of the century this subject was studied more especially with regard to the distribution of plants, and their relation to soil and climate; but since the publication of the 'Origin of Species' the purview has been greatly extended. It then became necessary to study the relation of plants, not only to inorganic conditions, but to each other and to animals; in a word, to study all the adaptations of the plant with reference to the struggle for existence. The result has been the accumulation of a vast amount of most interesting information. For instance, we are now fairly well acquainted with the adaptations of water-plants (hydrophytes) on the one hand and of desert-plants (xerophytes) on the other; with the adaptations of shade-plants and of those growing in full sun, especially as regards the protection of the chlorophyll. We have learned a great deal as to the relations of plants to each other, such as the peculiarities of parasites, epiphytes, and climbing plants, and as to those singular symbioses (Mycorhiza) of the higher plants with Fungi which have been found to be characteristic of saprophytes. Then, again, as to the relations between plants and animals: the adaptation of flowers to attract the visits of insects, first discovered by Sprengel (1793), has been widely studied; the protection of the plant against the attacks of animals, by means of thorns and spines on the surface, as also by the formation in its tissues of poisonous or distasteful substances, and even by the hiring of an army of mercenaries in the form of ants, has been elucidated; and finally those cases in which the plant turns the tables upon the animal, and captures and digests him, are now fully understood.

CONCLUSION.

Imperfect as is the sketch which I have now completed, it will, I think, suffice to show how remarkable has been the progress of the science during the nineteenth century, more particularly the latter part of it, and how multifarious are the directions in which it has developed. In fact Botany can no longer be regarded as a single science: it has grown and branched into a congeries of sciences. And as we botanists regard with complacency the flourishing condition of the science whose servants we are, let us not forget, on the one hand, to do honor to those whose life work it was to make the way straight for us, and whose conquests have become our peaceful possession; nor, on the other, that it lies with us so to carry on the good work that when this Section meets a hundred years hence it may be found that the achievements of the twentieth century do not lag behind those of the nineteenth.

S. H. VINES.

*THE METHOD OF TYPES IN BOTANICAL NOMENCLATURE.**

FOR many decades the systematic botany of the United States can scarcely be said to have had a history separate from that of Europe, so extensively were our treasures exploited by transient visitors, while resident students of the science long remained

* Read at the New York meeting of the Botanical Club of the A. A. A. S., through the kindness of Mr. Charles Louis Pollard. On motion the paper was referred to the Committee on Nomenclature and the author was requested to offer it for publication in SCIENCE.

dependent upon European patrons and correspondents. But even after a considerable independent development had been reached in this country, botany remained centralized to the extent that the writings of a very few masters constituted a large percentage of the published output of the science, and scarcely less in America than in England was the taxonomic side dominated by the spirit and methods of the brilliant coterie of Kew systematists. It was inevitable, however, with the spread of scientific knowledge and the quickening of interest in biological subjects, that the time should come when systematic activity could be confined no longer to a few herbaria, when botany like other sciences must be decentralized. Though this fact has been deplored, especially by those who had enjoyed a more or less complete monopoly of opportunity, it must be admitted that scientific study is one of the natural rights of man about which no artificial barriers can be maintained. Moreover, systematic botany reached a stage when it became evident that the last word could not be spoken from the herbarium, and that the results of local field study are legitimate subjects for record and publication. As long as a few men contented themselves with the issue of a few large treatises per decade, inequalities in their taxonomic views or methods of nomenclature caused comparatively little difficulty, each generation following without serious confusion the recognized authority of its time. But as workers multiplied, the annoyances of contemporary differences became so great that the desire for uniformity gradually crystallized into a movement for the formulation of a rational code of nomenclature by which all might be guided.

As often happens in reform movements, a single issue became prominent, and attention was chiefly directed to the correction of what had come to be regarded as a flagrant and unreasonable abuse of the power of arbitrary change of names. The proposition known as the 'Kew Rule,' to the effect that a species might be renamed whenever transferred to another genus was emphatically negatived in the interest of a consistent application of the principle of priority. This does not mean that such a rule was essentially illogical, any more than was the other custom of eighteenth century botanists who set aside by wholesale the genera of their predecessors, substituting their own improved concepts and more euphonious names. Neither was the changing of specific names anything new; it had been customary throughout the history of systematic botany, but the time had passed when the scientific public could be trifled with, even by the specialist sure of the finality of his own conclusions.

In spite of minor features which still seem objectionable to many botanists, such as the supplanting of specific names by varietal, and the use of duplicate binomials, the 'Rochester Rules' have proved to be a most valuable piece of progressive legislation, the general wisdom and logical authority of which it is not necessary to question. At the same time it is unfortunate that many seem to have expected the new code to be final and perfect, even in matters which did not come before the minds of those who prepared it, but a disappointment in this regard should be no real hindrance to the consideration of other possible improvements in nomenclatorial procedure. Such finality of creeds is scarcely to be expected in progessive sciences, notwithstanding the eminent desirability of permanence and uniformity. The Rochester Code affirms the supremacy of the principle of priority and provides for its universal application in the nomenclature of species. The successful initiation and satisfactory progress of this measure but makes plainer the need of a similarly

salutary regulation for determining the application and precedence of generic names. Although sometimes believed to have been adequately dealt with, this question was only indirectly touched upon by the Rochester Rules, which simply re-enacted by implication the generally neglected provisions of the Paris Code of 1867. This legislation can no longer be considered authoritative, since it was based on the pre-Darwinian doctrine that species are special creations and that the categories of classification are mere mental concepts, instead of groups of individuals having a common origin and phylogenetic relationships. As a concept, there is no particular reason why a genus should not be emended, subdivided or set aside entirely if found erroneous, but as a group of related species for which a permanent common name is desired, the genus should no longer be treated by the formal or conceptual method. Obviously, it is far more important, as well as more scientific and more practical, that a part of organized nature have a fixed designation than that naturalists continue to waste their energy in investigating the applicability and adjusting the claims of the varied succession of rival concepts. Although to many the genus appears to be less tangible than the species, it is possible to guarantee to it the permanence and stability now enjoyed by the species under the Rochester Code. By considering a single species the nomenclatorial type of its genus, to which the name is to remain inseparably attached, we place upon firm ground and solidify to the point of general tangibility and comprehension the misty fabric of conceptual classification.

At the Springfield meeting of the Botanical Club where the legislation begun at Rochester was concluded by the acceptance of the report of the Nomenclature Committee, an attempt was made to secure attention for this matter of definite priority for genera by the recognition of a method of fixing the types. The necessity of some such procedure in carrying out a satisfactory revision of at least one group of organisms was explained in a paper entitled 'Personal Nomenclature in the Myxomycetes.'*

It appeared, however, that those who had been most zealous for the reform of specific nomenclature had not the same appreciation of the problems of generic taxonomy, perhaps because the illogical and unstable results of the method of concepts are less obvious in dealing with the higher plants, and especially with the European and North American floras in which the species of the older writers are nearly always identifiable, at least to the extent of determining their generic relationships. It is thus usually *possible* to apply the so-called method of residues or elimination under which the type species or a genus are held to be those of the original complement which have not been removed. But by this rule it is often quite impossible to fix the application of a generic name to one group of species when several were enumerated under the generic name at its first appearance. Thus if the three original species of a genus are found to belong to as many natural groups the decision as to which shall have the use of the name often depends, in final analysis, not upon anything which can be learned by consulting the original or subsequent descriptions, or even the type specimens, but

*Subsequently published in the *Bulletin of the Torrey Botanical Club*, Oct. 1895, xxii, 431–434. The present and related questions of taxonomy have also been discussed under these titles: 'Stability in Generic Nomenclature,' SCIENCE, Aug. 12, 1898, viii, 186–190, 'The Method of Types,' SCIENCE, Oct. 14, 1898, viii, 513–516, and 'Four Categories of Species,' *American Naturalist*, April, 1899, xxxiii, 287–297. In his 'Review of the Genera of Ferns proposed prior to 1832,' *Memoirs of the Torrey Botanical Club*, Dec. 1899, vi, 247–283, Professor Underwood has restated and applied the method of types, with exceptions required by the present limitations of the Rochester Rules.

upon the present monographer's views as to the relationship of the species with others included under other concepts named by writers previous or subsequent to the date of the genus under investigation. Thus, to take a very simple case, if there were a genus *A* described in 1830 with three species of which *a* is nearest related to *d*, of genus *B*, 1840, *b* is nearest related to *e*, of genus *C*, 1820, while *c* is nearest related to *f*, of genus *D*, 1850, we have already under the method of elimination a series of varying alternatives:

1. If the genera *B* and *C* be deemed valid, *D* cannot be separated, but is considered synonymous with *A*.

2. The systematist who decides that *B* is invalid applies *A* to *a* and *d* and may recognize *D* as a good genus.

3. If *C* be treated as invalid *A* may be applied to *b* and *e*, *B* and *D* being considered good.

Thus while it may be theoretically possible for a monographer to arrange to his own satisfaction the relations of the different genera, a change of taxonomic opinion affects not only the supposed limits of the genera but may *necessitate* a totally different application of the name *A* to any one of the three groups of species. And when we reflect that the complications are increased in almost geometrical ratio when the species are more numerous and when the question of the validity of *B*, *C* or *D* may be subject to equally great complications from other aspects of their real or supposed relationships, it becomes evident that the conceptual method of elimination involves an endless chain of casuistry, and is a counsel of darkness and confusion rather than of stability and perspicacity. Moreover, in the lower plants and animals the large composite genera of the earlier writers are in many cases now distributed, not merely to different families, but even to different orders and classes, so that the elucidation of some of the more difficult cases of residual taxonomy would require months of unprofitable labor in different parts of the biological field, and yet the conclusions could have only individual sanction, no steps in the process being secure with the exception of those which deal with genera described as monotypic. The designation of type species by a simple and uniform method would, however, render the application of all generic names equally definite, and would largely eliminate the personal equations which have thus far added immeasurably to the labor of biologic taxonomy, and which continue to hamper all efforts to popularize the science.

Although, as previously noted, the Rochester Rules gave a tacit adherence to the method of elimination, the case is not, in reality, that of supplanting one method of procedure by another, since with the possible exception of a small proportion of the flowering plants the method of elimination has never been consistently applied in any part of the botanical series. Most botanists, Continental, English and American, have continued to deal with genera in a manner purely personal and arbitrary. Seldom has there been any formal recognition of a type much less the choice of one by any fixed rule. Genera have often been deprived of all their original species and made to do duty for an entirely new set, with or without modification of the original description.

The conditions obtaining in the earlier genera of ferns have been investigated by Professor Underwood, and found to be much the same as in the Myxomycetes and Fungi, while a brief excursion among the palms reveals the persistence there of the spirit of lawlessness. The genus *Oreodoxa*, for example, was based on two species, one of which is now placed in *Euterpe*, and the other in *Catoblastus*, while the name *Oreodoxa* has been applied without warrant to the

royal palm and its allies, which have never been designated by a correct generic name,* whether the difficulty be adjusted by the method of elimination or by the method of types. Of course it is not necessary that the types of phanerogams should be fixed by the same method as in the other groups, but all phanerogamists are not likely to remain contented with an illogical and faulty method, and it is scarcely to be expected that the Committee on Types appointed at the Buffalo meeting, will bring in recommendations for a variety of usage in a matter of so much importance.

In the incorporation of the desired legislation into the Rochester Code a large variety of courses might be followed, but for present purposes it may be sufficient to point out that these lie between two general policies, either of which may be developed in such form as to be both logical and practical. If we adhere strictly to the binomial system, to 1753, and to the '*Species Plantarum*,' we must reconcile ourselves to the misapplication of the pre-Linnæan names or treat them as exceptions and provide for the assignment of types by a committee or a congress, thus disposing at once of many bibliographic complications. This would be in accordance with the argument advanced by some of the advocates of the Rochester Code, that the process of revision of cryptogamic as well as of phanerogamic genera would be greatly simplified by relief from the incubus of the pre-Linnæan and non-binomial literature, an expectation which undoubtedly influenced many in favor of that legislation. It transpired, however, that instead of adhering to the logical consequences of the adoption of a nomenclature of genera and species based on the binomial system with the '*Species Plantarum*' as a starting point, the very committee which had framed the rules fell into the practice of interpreting Linnæus through the works of his predecessors instead of establishing the usage and identifications of his followers, thus rendering the date 1753 merely an arbitrary limit for citations, and virtually abandoning all the advantages which might have been secured by a consistent adherence to the original import of the Rochester Code, as far as it affected the taxonomy of genera. Moreover, in addition to the re-introduction of this complication, there was unearthed a large body of irrelevant, non-binomial literature issued subsequent to 1753, much of which had rested in merited oblivion for upward of a century. To accept as taxonomic literature such writings as those of Adanson, while refusing to cite Tournefort and Micheli, destroys every rational or practical effect of the intended reform and reduces the result of the Rochester legislation, as far as genera are concerned, to the empty absurdity of requiring the false citation of Linnæus and Adanson as the authors of genera which they knew only as compilers from the works of older and better botanists.

It is plain, therefore, that any argument which might have been drawn from the fact of previous legislation, if it had been logically carried out in this respect, has been lost by the apparently unconscious surrender of the Rochester Code reformers to Professor Greene's contention for the recognition of the pre-Linnæan authors, and we may thus without prejudice consider the second of the available alternatives for the enactment of a law for fixing generic names by types. To abandon 1753 as the initial date for generic nomenclature is but frankly to admit what is already an accomplished fact, and to cease to quote Linnæus, Adanson and others as the authors

*A new genus *Roystonea* is proposed, differing from *Oreodoxa* in the solitary growth, the double spathe and other characters. The type is *R. regia* (HBK), Nov. Gen. et Sp. 1 : 305, originally described from Cuba.

of genera which they did not discover. Such a step need not, however, compel us to return to the Middle Ages or to Classical Antiquity; Tournefort's '*Institutiones*' published at the appropriate date 1700 was an important integration of previous knowledge, and has long been considered the beginning of modern botanical literature; beyond this our taxonomy scarcely needs go to. Commencing with the 'Father of Genera' the selection of the first species as the type would result in no complications by reason of the Linnæan arrangement of species, and it may be confidently expected that the uniform application of such a rule would necessitate far fewer changes than would the method of elimination, whereby the doubtful or unidentifiable species are often the only residue on which time-honored names could be maintained.

To many who have desired to minimize as far as possible the bibliographic labor which is so great a burden to systematic botany, the adoption of such a change will be a matter of regret, but this argument cannot be used by the authors of the 'Check List' and other publications prepared on the basis of the Rochester Rules, since these have cheerfully assumed the burdens and multiplied the changes which a closer adherence to the binomial system would have avoided. And yet the task is quite finite, especially since we should be under no obligation to attempt the re-identification of the pre-Linnæan species, but may infer most of them with historical warrant from the citations of '*Species Plantarum*' and subsequent binomial literature.

Choice lies thus between the restriction of taxonomic recognition to genera provided with a binomial species in '*Species Plantarum*' or some subsequent work, or the admission of the genera of Tournefort and his successors whenever referable to an identifiable species, whether binomial or not. While it is true that these alternatives could be combined or modified in a variety of ways, such compromises could result only in exceptions and complications which experience has shown to be held in small favor by those who do not oppose change merely from motives of inertia.

A justification for a *laissez faire* policy in nomenclature is often based on the allegation that since the species and other categories of classification cannot be accurately defined and equalized there is no possibility of the attainment of either uniformity or stability in the use of names. Whatever may have been the justice or the logical propriety of this destructive criticism as applied to a taxonomic system based on the method of concepts, it is purely specious and ineffective with reference to the method of types. The species is a group of individuals, the genus a group of species, the family a group of genera, and these terms are quite as definite and comprehensible as other collective nouns. Botanists may never agree on the number of species, or on the number of groups of species which should be recognized as genera, but it is entirely possible for them to agree on the names as far as they agree on the groups, not by deferring to arbitrary authority, but by adherence to a rational and uniform course of procedure. As long as a genus is viewed as a concept, it belongs, obviously, where it fits best, and it is quite logical to reject it if no correspondence in nature be found, or to move it along to new series of species, where the description is more applicable than to those for which it was drawn. The conceptual theory of taxonomy comported entirely with the doctrine of special creation, but it is not adapted to the purposes of phylogenetic classification as an integration of the results of the study of the evolution of organic types, and its continued use is now unscientific as well as unpractical. As the genus does not consist

of a concept, neither can it become adequately known to us through the medium of description. Botany without designation of types is like geography without position.

In biology a species is a coherent or continuous group of organisms. In such a group the individual organisms have a common origin and may be arranged in connected series of imperceptible gradations with reference to any one character, except in cases of sexual differentiation and alternation of generations, where the coherence of specific groups is maintained by facts of life-history. A species is not constituted by any antecedent determination of the amount of difference it must present; it subsists in virtue of the fact that it has diverged and become disconnected in nature from other groups of organisms, however similar these may be.

For nomenclatorial purposes a species is a group of individuals which has been designated by a scientific (preferably a Latin adjective) name, the first individual to which the name was applied constituting the type of the species. The importance of preserving type specimens with special care is now recognized throughout the scientific world, and where specific types are lacking, naturalists are endeavoring to supply their place by specimens collected in the original localities. This may be taken as a general admission of the obvious fact that purely descriptive methods are generally insufficient for scientific accuracy and need to be supplemented by actual specimens if correct identifications are to be permanently assured.

For purposes of reference and citation specific names which appeared previous to the 'Species Plantarum' of Linnæus are not regarded in botanical nomenclature. In reality Linnæus revived rather than originated the binomial system of nomenclature, but his works embody the results of the first extensive and fairly consistent attempt at the scientific application of the nomenclatorial practice now universally followed.

The method of types applied to genera involves a similar readjustment of views. Under the analytic method of concepts a genus has been defined as a sub-division of a family, but the method of types is synthetic and places the emphasis on the connection with nature by building the genus up from below.

A genus of organisms is a species without close affinities, or a group of mutually related species. Here again the natural arrangement must have reference to the gaps in nature rather than to the logical balance of formal characters.

A generic name is established in taxonomy when it has been applied to a recognizable species. Unless the discoverer of the genus designates a type species in the same publication in which he bestows the name, the first species referred to the genus should serve as its nomenclatorial type.

The generic taxonomy of plants may be treated as beginning with Tournefort's 'Institutiones' (1700).

O. F. COOK.

WASHINGTON, D. C.

SCIENTIFIC BOOKS.

Memoirs presented to the Cambridge Philosophical Society on the occasion of the jubilee of SIR GEORGE GABRIEL STOKES, Bart., Hon. LL.D., Hon. Sc.D., Lucasian Professor. Cambridge, at the University Press, 1900; New York, The Macmillan Co. 4to. Pp. xxviii + 447, with 25 plates. Price, $6.50.

The celebration of the fiftieth anniversary of the Lucasian professorship of Sir George Gabriel Stokes at the University of Cambridge, on June 1 and 2, 1899, brought together a large number of distinguished naturalists, if one may use this convenient term to include astronomers, chemists, geodesists, geologists, mathematicians, physicians, physicists and zoologists. It was one of those occasions which illustrate the essential unity of science by a spontaneous tribute of homage to an eminent specialist from workers in widely divergent fields. During the week following the